

# New classical/real business cycle macroeconomics The anatomy of a revolution

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# New classical/real business cycle macroeconomics. The anatomy of a revolution

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## Abstract

The aim of the present paper is to assess the new classical/real business cycle revolution, which dethroned Keynesian macroeconomics. In its first part, I critically discuss the microfoundations requirement that constitutes a cornerstone of the new approach and suggest an alternative, softer, formulation of it. The conclusion of this discussion is that the new classical/real business cycle revolution marked a transition from a soft to a demanding understanding of the microfoundations requirement. In the second part of the paper, I present additional salient traits of the new classical and the real business cycle stages of the revolution. While each of these stages brought a specific contribution to the revolution, I emphasize the decisive role played by Kydland and Prescott in re-orienting the type of work in which macroeconomists were engaged. Finally, in part three, I ponder upon the causes of this revolution. After presenting and assessing Prescott's and Lucas's accounts of the factors which gave rise to the new approach, I venture into muddier waters by raising the question of whether a political agenda underpinned the NC/RBC revolution.

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## INTRODUCTION

As will be documented in this paper, there are good reasons for viewing the transition from Keynesian IS-LM macroeconomics to dynamic stochastic macroeconomics as a scientific revolution *à la* Kuhn. This expression refers to an episode in the history of a given discipline where a period of normal science is disturbed because of the persistent existence of apparently unsolvable puzzles and a drive to move the agenda and the research methods into new directions. This goes along with thundering declarations of war — Keynesian theory is dead — a confrontation between the young and the old generation, the rise of new stars in the profession and the eclipse of old ones. The relevance of the concept of a scientific revolution hinges on the existence of a ‘before’ and an ‘after’, with a well delineated series of events in between, so that the type of work members of the community are engaged in after the revolution bears little resemblance to earlier practices.

This revolution — at present often viewed as having led to the rise of DSGE (dynamic-stochastic general equilibrium) macroeconomics — occurred in two stages. The first is associated with Robert Lucas’s work and has often been labeled the ‘new classical revolution’. The second is associated with Kydland and Prescott and real business cycle models. To capture this two-step process, I shall refer to it as the new classical/real business cycle revolution (henceforth NC/RBC) rather than the DSGE revolution.<sup>1</sup> In a nutshell, Lucas did the job of attacking the Keynesian paradigm and of introducing a series of new concepts and principles. Kydland and Prescott’s transformed Lucas’s qualitative modeling into a quantitative research program — as Greenwood ([1994] 2005, p.1) put it, they took macroeconomics to the computer. Of course, many other researchers played an important role in this revolution, in particular Sargent, Barro, Wallace, Plosser and Long. Lucas, and Kydland and Prescott have nonetheless been its towering figures, with Kydland playing a more subdued role in the Kydland and Prescott duo, at least as far as the defense of their joint work is concerned.

Several questions arise: What were the causes of the NC/RBC revolution? What changes did it bring about? Did it have a political dimension? These are the issues I wish to tackle in this paper. It comprises three parts. The NC/RBC revolution is often presented as having consisted of giving macroeconomics the microfoundations it lacked. I appraise this characterization in the first part of the paper. In the second, I bring out the different points on which the new way of doing macroeconomics differed from the old one. Finally, in part three, I ponder upon the causes of this revolution. I start with a discussion of Prescott’s and Lucas’s accounts of the revolution, after which, entering into more troubled waters, I raise the question of whether there was a political agenda underpinning the NC/RBC revolution.

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<sup>1</sup> An additional reason for not using the DSGE terminology is that, with the appearance of new neoclassical synthesis models, it extends beyond real business cycle models.

## A MATTER OF MICROFOUNDATIONS?

### A new methodological requirement

From the 1970s onwards, a new methodological principle came to prominence in macroeconomics, the microfoundations requirement. It became the *sine qua non* of valid theoretical practice: the condition for a macroeconomic model to be microfounded is that it starts with the description of how agents make their choices, these being made in an optimizing way. An objective function is to be maximized or minimized under given constraints. For all its generality, this condition is nonetheless deemed sufficient to identify models that do not accord with it, and thence ought to be rejected.<sup>2</sup>

The same requirement has been expressed differently by Lucas and Sargent (and Lucas on his own) under the name of ‘equilibrium discipline’. It states that, to be valid, economic models should rest on two postulates: (a) that agents act in their own self-interest and their behavior is optimal; and (b) that markets clear (Lucas and Sargent [1979] 1994, p. 15). The ‘discipline’ term is used to convey the view that this is a rule that economists impose upon themselves, and which stamps their specific way of looking at social reality. Accepting such a standpoint results in proclaiming that the notion of disequilibrium, which before was widely used, should be banned from the economic lexicon.

Two additional remarks are worth making. First, the insistence on the microfoundations requirement did not stem from microeconomists wanting to assess the good practice of macroeconomists from the standpoint of their own sub-discipline. Rather, it originated from within the macroeconomic community, the result of a gradual awareness by some of its members that something was wrong with the existing practice of the discipline, and that this malaise had to do with its drift away from microeconomics. Actually, new classicals were not the first macroeconomists to be preoccupied with microfoundations. It all started with Keynesian economists, such as Patinkin (1965), Clower ([1965] 1984), and Barro and Grossman (1971), who set themselves the task of improving Keynesian theory by giving it more rigorous foundations. It is only in a second stage that the microfoundations requirement came to be associated with external criticism of Keynesian theory. Second, it is sometimes claimed that new classicists invented market clearing. This is not true. Market clearing (i.e. the idea that supply and demand always match in a given period of exchange) is of long standing in economics. Its presence in Walrasian theory is beyond dispute. But the same is

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<sup>2</sup> The intuition behind the requirement is well expressed in the following quotation from an interview with Lucas: “I think a lot of the work in Keynesian economics has gotten too far away from thinking about individuals and their decisions at all. Keynesians don’t often worry about what actual individuals are doing. They look at mechanical statistical relationships that have no connection with what real individuals are actually doing” (Lucas 1989).

true for Marshallian theory (with the additional complication that market clearing and disequilibrium can coexist, see De Vroey, 2007). By challenging this consensus in the profession, Keynes was clearly thinking out of the box. Thus, rather than having invented market clearing, new classicists have just restored it at a higher level, signaling the end of the Keynesian recess. Still, the pace at which the microfoundations requirement conquered the profession is impressive. More curiously, this conquest occurred without any justification being provided, as if the case was so obvious that none was needed.

### **Lucas's criticism of Keynesian theory and modeling**

The gist of Lucas's criticism of Keynesian theory is that it does not abide by the equilibrium discipline. His attack develops at two levels. The first pertains to the general way in which Keynes addressed the issue of unemployment in his *General Theory*. In Lucas's eyes, the mere aim of wanting to produce a theory of involuntary unemployment constitutes an infringement of the equilibrium discipline (Lucas [1977] 1981).

After freeing himself of the straightjacket (or discipline) imposed by the classical postulates, Keynes described a model in which rules of thumb, such as the consumption function and liquidity preference schedule, took the place of decision functions that a classical economist would insist be derived from the theory of choice. And rather than require that wages and prices be determined by the postulate that markets clear — which for the labor market seemed patently contradicted by the severity of business depressions — Keynes took as an unexamined postulate that money wages are sticky, meaning that they are set at a level or by a process that could be taken as uninfluenced by the macroeconomic forces he proposed to analyze (Lucas and Sargent [1979] 1994, p. 15).

Keynes's lapse from the equilibrium discipline, Lucas is ready to admit, is understandable in view of the apparent contradiction between cyclical phenomena and economic equilibrium in the context of the Great Depression. Still, *ex post* it ought to be interpreted as having prompted a long detour in the progress of economic theory. It is an example of “bad social science: an attempt to explain important aspects of human behavior without reference either to what people like or what they are capable of doing” (Lucas, 1981, p. 4).

The second level of criticism is the well-known ‘Lucas critique’ ([1976] 1981). Here his target is the macroeconometric models of the time, all of which had a Keynesian inspiration. Lucas's claim is that, although they do a fairly good job of forecasting, these models are a failure as far as the assessment of alternative policies is concerned. Their main flaw is their lack of microfoundations. This leads to endogenous variables, sensitive to variations in economic policy, being transformed into exogenous ones. As a result, a model of the economy estimated at a period during which a particular institutional regime holds sway will provide inadequate information for assessing what might occur under a different regime.

According to Lucas, to avoid this defect, the parameters of the model need to be ‘deeply structural’. That is, they must be derived from the fundamentals of the economy, agents’ preferences, and technological constraints.

### **Assessing the equilibrium discipline principle**

Before appraising the validity of Lucas’s twofold criticism, it is worth pondering upon the equilibrium discipline as posited by Lucas and Sargent, independent of its use as a weapon against Keynesian theory. It is a fact that, since the inception of political economy, the equilibrium notion has played a central role in it. So the idea of equilibrium discipline as the hallmark of economics makes sense. However, I am unconvinced by the way in which Lucas and Sargent conceive it. First of all, contrary to what they say, it actually contains only one criterion. Optimal behavior and market clearing are two faces of the same coin. They grasp the same reality at two distinct levels: optimal behavior refers to individual or personal equilibrium, while market clearing relates to what could be called ‘interactive equilibrium’, a state where all individual optimal plans have been made compatible. Moreover, their conception hides under the rug a distinction that I, for one, find crucial. It was expressed long ago by Hayek and Patinkin but subsequently felt into oblivion:

I have long felt that the concept of equilibrium itself and the methods which we employ in pure analysis have a clear meaning only when confined to the analysis of the action of a single person and that we are really passing into a different sphere and silently introducing a new element of altogether different character when we apply it to the explanation of the interactions of a number of different individuals (Hayek [1937] 1948, p. 35).

A similar insight is to be found under Patinkin’s name when he draws a distinction between individual experiments and market experiments (1965, pp. 11–12 and 387–392).<sup>3</sup> It follows from Hayek’s and Patinkin’s standpoints that the notions of the *optimizing planning* and *optimizing behavior* designate different realities. Optimizing planning refers to agents’ intentions as existing before the opening of trading, the solution to the choice-theoretical

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<sup>3</sup> Yeager aptly commented on this distinction: “An individual experiment involves discovering, at least conceptually, the desired behavior of an individual person, of a small or large group of individuals, or even of all individuals in the community, acting in certain capacities, under certain specified circumstances. Whether these circumstances are compatible with other economic conditions and whether they can in fact prevail (whether they are genuinely or even conceptually attainable, to use the Chicago terminology) is beside the point: it is not the purpose of an individual experiment, by itself, to describe the economic equilibrium that will tend to emerge. ... This other type of analysis, which pulls together the results of various individual experiments, examines the conditions under which the plans of various persons would and would not mesh, describes the processes at work when plans fail to mesh, and describes the equilibrium position, is what Patinkin means by market experiments” (Yeager 1960, p. 59).

problem they are faced with.<sup>4</sup> Optimizing behavior refers to what is observable after trading has started. Thus, optimizing behavior implies that the optimizing plan has been realized. The gist of the above quotations is that optimal plan and optimal behavior need to be logically separated — there is a difference between finding a solution to a choice problem and implementing this solution. Whenever optimizing behavior is the sole concept used, the possibility of there being a difference between them is discarded by definition.

This difference can also be expressed with reference to the notions of equilibrium and disequilibrium. Individual equilibrium is a state where an agent is able to achieve one element of his or her optimal plan. Individual disequilibrium refers to the opposite, the inability of some agents to have their optimal plans transformed into optimal behavior. Note that this notion does not run counter to the view that agents are rational and develop optimal plans. Equilibrium *tout court* is what I labeled interactive equilibrium above. “The general equilibrium implies that all subsets of agents are in equilibrium and in particular that all individual agents are in equilibrium” (McKenzie 1987, p. 498). That is, interactive (or market) equilibrium requires individual equilibrium. McKenzie’s quotation confirms my view that optimizing behavior and market clearing are one and the same thing. Symmetrically, in the conception that I defend, optimal plan and market clearing are distinct while market non-clearing and individual disequilibrium go hand in hand.

It remains to assess whether the two distinct potential criteria for the microfoundations requirement, optimizing planning and market clearing, are valid candidates for this function. Adopting optimal planning as a postulate raises no serious objections. It amounts to assuming that agents have the ability to optimally solve any decision problem they encounter (with the ensuing correlates of rationality, information and rational expectations). This assumption is certainly an exaggeration, yet it is nonetheless acceptable as it is probably better, and certainly more tractable, than alternative assumptions.

But the same is not true for the second component, market clearing. I see no reason to adopt it as a condition for sound economic reasoning. Market clearing is the consequence of some prior assumptions related to ‘trade technology’, i.e. the institutional set-up that is needed to make the realization of equilibrium possible. Like other Walrasian models, Lucas’s models are based on the *tâtonnement* or auctioneer hypothesis. This theoretical scenario explains how the equilibrium values calculated by the economist when studying the logical existence of a general equilibrium could come into existence in the artificial economy described by the model. As soon as this hypothesis is made, the matter is sealed: market clearing always occurs. Thus, market clearing is the direct consequence of the auctioneer hypothesis rather

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<sup>4</sup> In Patinkin’s words: “We can consider the individual — with his given indifference map and initial endowment — to be a ‘utility-computer’ into whom we ‘feed’ a sequence of market prices and from whom we obtain a corresponding sequence of ‘solutions’ in the form of specified optimum positions” (1965, p. 7).

than a consequence of self-interest and rationality. The problem with the auctioneer hypothesis is that it runs counter to the essential nature of the theory's theoretical *explanandum*, because it amounts to picturing a decentralized system as a centralized organization of trade.<sup>5</sup>

To conclude the discussion of this point, claiming that macroeconomics ought to be based on the microfoundations requirement is fine by me, in so far as this requirement is understood as just stating that agents ought to be modeled as elaborating optimizing plans before entering into exchange relations. This is a softer, less stringent definition than Lucas and Sargent's. In particular, no a priori ban is put on the notion of individual disequilibrium.

### **An appraisal of Lucas's two criticisms**

My reconsideration of the equilibrium discipline leaves Lucas's critique intact. There is no need to revise it since it bears on agents' devising of their optimal plans. If the rational expectations assumption is accepted, Lucas is right to claim that agents should change their optimal plans whenever the policy regime is modified.<sup>6</sup>

Lucas's more general criticism of Keynesian theory is different. If my understanding of equilibrium discipline is adopted, his criticism falls, and so the ban on individual disequilibrium ought to be removed. Keynes's project of constructing a theory of involuntary unemployment as a case of individual disequilibrium is compatible with my conception of the microfoundations requirement. Actually, several elements testify that Keynes reasoning was based on microfoundations: he assumed that firms were maximizing profits — the marginal efficiency of capital is a scheme enabling outlays in new capital to abide by the maximization of profits purpose. Keynes also assumed that households were arbitraging between bonds and liquidity. The definition of involuntary unemployment in Chapter Two of Keynes's *General Theory* is a case of agents being unable to make their optimizing plan come through, which implies that agents are devising optimal plans. Admittedly, these were just sweeping assertions in Keynes's writing. But then he was a Marshallian, and followed Marshall in asserting that supply and demand functions were choice-theoretically underpinned without bothering to develop this point fully. Nothing as elaborate as microfoundations *à la* Walras can be found in Keynes's work.

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<sup>5</sup> See De Vroey (1998) for further discussion of this point.

<sup>6</sup> However, the first model builders deserve some indulgence because of the undeveloped state of technique at the time. Klein anticipated the need for microfoundations, as is clear from the first book he wrote for t Lucas [1973] 1981) he Cowles Foundation, *Economic Fluctuations in the United States, 1921-1942* (Klein [1950] 1964). New classicists could subscribe to the program he set out there. However, when, in his joint work with Goldberger, *An Econometric Model of the United States 1929-1952* (Klein and Goldberger, 1955), he came to setting out a full-blown model, he had to back down and adopt pragmatic solutions. Moreover, at the time nobody was aware of the drawbacks of using backward- rather than forward-looking expectations.



To add a final note on this point, looking at Keynes's non-theoretical writings, I have found in his correspondence a passage that Lucas could have written in support of his *Critique*. It relates to the discussion of Tinbergen's work.

I also want to emphasize strongly the point about economics being a moral science. I mentioned before that it deals with introspection and with values. I might have added that it deals with motives, expectations, psychological uncertainties. One has to be constantly on guard against treating the material as constant and homogeneous. It is as though the fall of the apple to the ground depended on the apple's motives, on whether the ground wanted the apple to fall, and on mistaken calculations on the part of the apple as to how far it was from the centre of the earth (Moggridge 203, p. 300).

So, with a little stretch of the imagination, we may attribute to Keynes the view that Tinbergen should have refrained from engaging in empirical work because this would breach the microfoundations requirement!

Hitherto, I have taken it for granted that the microfoundations requirement and the equilibrium discipline are one and the same thing. It now turns out that my reservations lie less with the microfoundations aspect (in as far as it is redefined in my way) than with the pervasiveness of equilibrium. As already claimed, I do not think that we should accept that, as a matter of principle, states of individual disequilibrium are expelled from the lexicon of authorized economic concepts.

But there is more to it than that. In De Vroey (2004b), I showed that neither Keynes nor his disciples achieved the program that was spelled out in the *General Theory*. Constructing a theory of involuntary unemployment, the emblematic case of individual disequilibrium, under the conditions set out by Keynes, has proven to be a daunting task, so daunting as to fall under the spell of the Wittgensteinian adage that one ought to be silent on issues about which one lacks the ability to speak properly.

In other words, there are grounds for arguing in favor of macroeconomics continuing to be based on the equilibrium discipline as conceived by Lucas, until a 'disequilibrium revolution' occurs. However, this line is commendable only as a solution of expediency, since it is due to macroeconomists' inability to conceptualize individual disequilibrium and market non-clearing in a rewarding and robust way. Lucas and Sargent's flaw is to have transformed this expediency into a virtue. Instead of boasting about the equilibrium discipline, macroeconomists should view it as a mark of their limited capability. To put it another way, my quarrel with NC/RBC economists here is less about their substantive position (i.e. the adoption of the equilibrium discipline), than about their meta-theoretical comments about it.

## Conclusion

Is it fine to characterize the NC/RBC revolution as having introduced microfoundations into macroeconomics? Yes and no! In my view, what occurred was a shift from a soft understanding of the microfoundations requirement — the injunction that agents ought to be depicted as elaborating optimizing plans — to a strong understanding enunciating that they must be depicted as behaving in an optimizing way. Keynes inaugurated the soft requirement line of research. Before him, the strong requirement prevailed unwittingly. Thus, the revolution amounted to the restoration of an earlier prevailing state of affairs. Be it only for this reason, the 'new classical' terminology is apposite: the old premises are reasserted in a more explicit and vigorous way.

## FURTHER CHARACTERIZATION OF THE NC/RBC REVOLUTION

As stated above, the NC/RBC revolution was a two-step process, the first being associated with Lucas (the new classical phase of the revolution), and the second with Kydland and Prescott (the real business cycle phase). Below, I will explore the changes in perspective that occurred in each of these two steps.

### The new classical phase

Lucas's role was twofold. First, treading in Friedman's footsteps, he launched an all-out attack on Keynesian macroeconomics. Positively, he introduced a series of new concepts and methodological perspectives. They were not necessarily his inventions — the obvious example is the notion of rational expectations, introduced by Muth — nor was he the only person to bring them to the forefront, but he did provide the impulse. The assumption of rational expectations and inter-temporal substitution are the cornerstones of the new approach (in addition to the equilibrium discipline). As these two aspects are familiar, I will focus more on some less well-known features of the new approach.<sup>8</sup>

### *Merging the fields of value theory and business cycle theory*

The NC/RBC revolution modified the boundaries between the economic sub-disciplines of value theory and business cycle theory, with business cycle theory being absorbed by value theory.<sup>9</sup> Before the revolution these fields were separate. Evolving at a high level of abstraction, value theory was based on trade technology or information assumptions resulting in the universality of market clearing. By contrast, business cycle theory consisted of qualitative, descriptive accounts of the unfolding of economies over time. Studies in this field aimed to provide specific explanations of fluctuations rather than a general theory. Although

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<sup>8</sup> For a pioneering, but still useful, assessment of the new classical approach, see Hoover (1988).

<sup>9</sup> In what follows, value theory, equilibrium theory and price theory will be considered synonymous.

the notion of equilibrium was only vaguely referred to, it was taken for granted that the cycle was a manifestation of economic disequilibrium.<sup>10</sup> Against this background, Keynes' aim in the *General Theory* may be reconstructed as an attempt to move unemployment away from the field of business cycle theory into that of value theory without implying that these two fields should be merged.

The new classical revolution took another path, to make the business cycle part of value theory while expelling unemployment altogether from the enlarged field. Two implications of this widening of the scope of value theory are worth mentioning. First, the idea now prevails that a theory of the business cycle can be constructed without resorting to the notion of unemployment, a view that was inconceivable before. The second implication is that the earlier judgments about the harmful character of business cycles were erroneous. Business cycles are no longer considered to be a manifestation of some malfunctioning. The earlier received wisdom that the state should intervene in order to mitigate fluctuations ceases to be valid.

#### *A change in the research agenda*

Nowadays, macroeconomics is loosely defined as the study of business fluctuations. But this was not true in the heyday of Keynesian macroeconomics. Then, unemployment, it was proclaimed, was macroeconomics' central object. Macroeconomics arose in the wake of the Great Depression from the wish to bring to the fore the existence of trading failures which, it was presumed, it was the role of the state to act upon. Small wonder that social reformers supported it. Unemployment was considered the main dysfunction. This state of mind was still present at the beginning of the 1970s.<sup>11</sup> But then, in a sweeping change, unemployment ceased to be an important preoccupation of macroeconomists. It fell out of fashion, macroeconomists being glad to send it back to labor economists.

#### *The replacement of Marshallian macroeconomics with Walrasian macroeconomics*

Elsewhere (De Vroey, 2004b) I have claimed that Keynesian macroeconomics ought to be considered as a simplified Marshallian general equilibrium theory. Admittedly, this is an unusual claim. Economists' reflex when seeing the term 'general equilibrium', is to associate it with Walrasian or neo-Walrasian theory as if these were the only ways of studying the

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<sup>10</sup> This split was already present in Marshall's work. Take his treatment of unemployment. No room for it existed in Marshall's value theory, its proper place being in business cycle theory. In Matthews's words (1990, p. 35), unemployment was *par excellence* a 'Volume II' subject.

<sup>11</sup> A testimony to this is Tobin's 1971 Presidential Address to the American Economic Association, in which he wrote that macroeconomics deprived of the concept of full employment was unimaginable (1972, p. 1), not realizing that such a state of affairs was just around the corner.

economy as a whole. To me, alternative approaches are conceivable, the Marshallian approach being the main one.<sup>12</sup>

The difference between the two approaches results from the consideration of various criteria: the purpose of economic theory; methodology (including the role of mathematics); the ways of looking at the working of the economy as whole; the conception of equilibrium underpinning the theories; and, finally, the trade organization assumptions (De Vroey, 2009c). Here, I want to focus on how Marshall and Walras tackled the issue of the working of the economy as a whole.

Marshall viewed the analysis of economics as so complex that he thought it wise to approach it gradually. To all intents and purposes, he divided the economy into industries, to be studied separately. Moreover, since the analysis of time was so tricky, he distinguished three time categories: the market day (the unit period of exchange); the short period; and the long period. This strategy amounted to postponing the study of the functioning of the economy as a whole (i.e. the piecing together of these partial results) to a stage at which enough separate understanding of its parts had been reached. Marshall made it his priority to study the particular rectangles in the upper part of Figure 1 (branches of the economy during a given time span) separately. The point was not that theory should be confined to the study of a single rectangle, it was rather that economists needed to proceed gradually. For example, in his fishing industry example, Marshall studied the gravitational process between market-day, short-period and long-period equilibrium within an industry — that is, in terms of the Figure, he looked at vertical connections. He also endeavored to construct horizontal connections. Except for his Note XXI in the *Principles*' Mathematical Appendix, he hardly entered into the study of the economy as a whole.

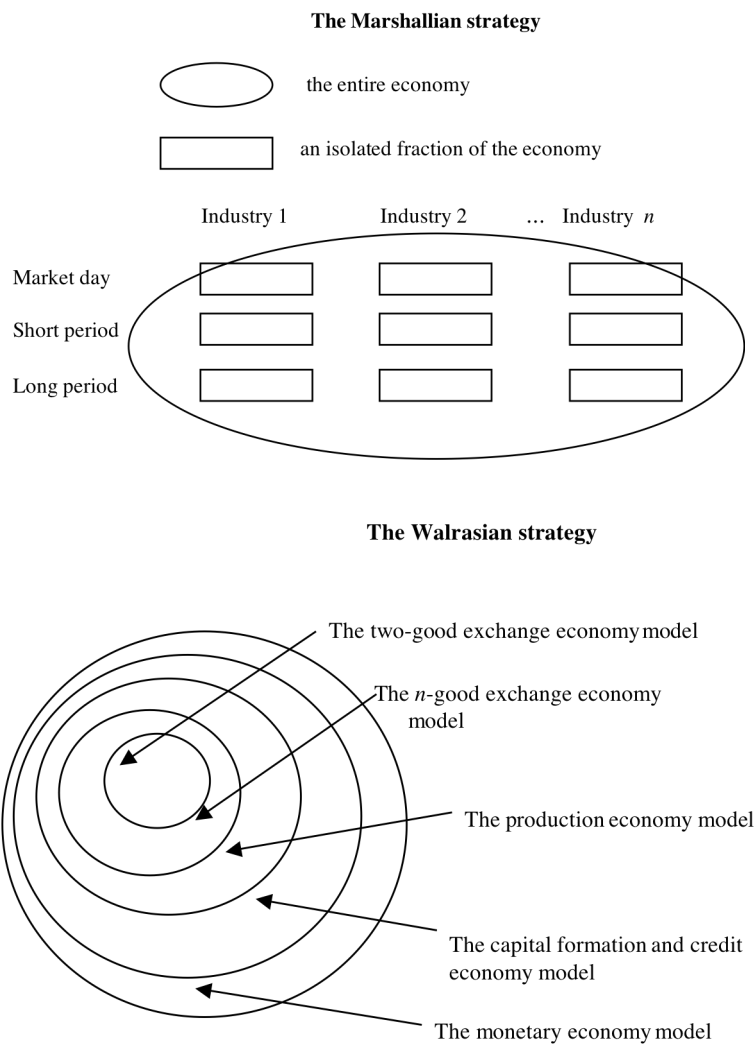
No such two-tier strategy is to be found in Walras's work. This is premised on the view that, from the onset, the object of study should be an entire economy. Simplifications had, of course, to be introduced, but they pertained to the characterization of the economy as a whole and did not involve dividing it into separate sub-entities. Walras inaugurated his analysis with the most rudimentary economy possible, a two-good exchange economy, where the two goods (oats and wheat) constituted the entire economy. He started by deriving the offer and demand curves from agents' optimal plans, in order to study the equilibrium of the economy next. This done, Walras moved on to consider a slightly more complicated economy, an  $n$ -good exchange economy. His next step was to introduce production into the picture. In the end, he had a chain of encompassing models, starting from the simplest and

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<sup>12</sup> Hicks wrote *Value and Capital* ([1939] 1946), a contribution to Walrasian theory, and the IS-LM model, a contribution to Marshallian theory, in roughly the same period. This led many commentators to believe that the IS-LM model was Walrasian, without investigating the matter further. In my opinion, it is Marshallian (see De Vroey 2004b).

moving towards greater and greater completeness: the two-good exchange-economy model, the  $n$ -good exchange-economy model, the production model, the capital-formation and credit-economy model and, finally, the monetary-economy model. Figure 1 illustrates Marshall's and Walras's strategies diagrammatically.

**Figure 1.** Marshall and Walras's models for simplifying the real economy



One rarely hears of Marshallian general equilibrium models. However, Keynesian macro-econometric models, as developed along the lines pioneered by Klein and Goldberg (1955) fit my description of a Marshallian economy remarkably well. In these models, the economy is sub-divided into separate sectors of activity. Initially these were limited in number, each being accounted for by a few equations: the consumption sector, the investment sector, the monetary sector, the employment sector, the government, and the international sector. Progress was seen as consisting of adding new equations to the models. It was assumed that these additions would make the model more descriptively accurate. They certainly made them bigger and bigger. Each sector became the object of a separate theoretical treatment.

Little consideration was given to overall consistency, that is to the piecing together of the various sectoral analyses.

All these traits place these models within a Marshallian representation of the economy. This was well perceived by the pioneers of the new classical approach. Look for example at the following passage drawn from an interview with Sargent. Although he does not mention either Marshall or Walras by name, what Sargent describes is nothing other than a shift from a Marshallian to a Walrasian framework:

The earlier literature proceeded as if you could build an optimizing consumption function, an optimizing investment schedule, an optimizing portfolio schedule, in isolation from one another. They are essentially partial equilibrium exercises which were then put together at the end. The Brookings model, built in [19]65, is a good example of this practice. They handed out these various schedules to different people and put them together at the end. The force of rational expectations is that it imposes a general equilibrium discipline. In order to figure out people's expectations you had to assume consistency (Klamer 1984, p. 66).

The Marshallian splitting up of the economy into separate sectors, each studied in isolation from the others, is absent from Walrasian macroeconomics. The latter witnesses a return to square one of Walras's program, the study of the simplest possible model economy (with, however, a few departures from Walras's original framework). Take Lucas's "Expectations and the Neutrality of Money" article (Lucas [1972] 1981). It features a three-good production economy ( $c$ ,  $c'$  and leisure), in which  $c$  and  $c'$  are physically identical yet are consumed at different dates. The physical good is non-storable. Because of the self-employment assumption, only two goods are traded at any time (leisure and either  $c$  or  $c'$ ): substitution is both intra- and inter-period. This is not a far cry from Walras two-goods exchange economy model.

### **The real business cycle stage**

Beyond doubt, there is a relationship of continuity between real business cycle models and Lucas's work.<sup>13</sup> They are based on the same conceptual ingredients: a perfectly competitive economy, the equilibrium discipline, rational expectations, a stochastic dynamic environment, and inter-temporal substitution. The three main departures Kydland and Prescott (1982) made from Lucas's approach were: abandoning his view of a money-driven cycle, an important move, which also meant a departure from the Friedmanian vision; replacing imperfect perfect information: engaging in applied work. This second change deserves further attention.

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<sup>13</sup> No surprise here. Lucas and Prescott were in close contact and had engaged in joint work, for example their 1971 paper, "Investment under uncertainty".

In his “Methods and problems in business cycle theory” paper, Lucas ([1980] 1981, p. 288) stated that the task ahead was to write a FORTRAN program. As he wrote in a related paper, the macroeconomist’s aim must be to construct “a fully articulate artificial economy which behaves through time so as to imitate closely the time series behavior of actual economies” (Lucas [1977] 1981, p. 219). However, Lucas himself contributed little to this enterprise. In contrast, Kydland and Prescott took Lucas’s injunction literally and devoted themselves to the task of transforming a qualitative type of modeling into a quantitative one (Woodford 1999, p. 25).

The work involved was titanic. To give the uninformed reader a taste of what it involved, let me survey the different steps that Kydland and Prescott took in their pathbreaking “Time to build and aggregate fluctuations” paper (1982).<sup>14</sup> Two parallel tasks were involved. On the one hand, work had to be done on real-world data, which often needed to be rearranged. On the other hand, the model had to be constructed and computed.

The first part of the first task was to gather growth data. At the start of their work, Kydland and Prescott also found it necessary to make the national accounting categories consistent with their theory. In their model, there was no government sector, no household production sector, no inventories, and no foreign sector. As a result, the data relating to these sectors needed to be reassigned. Moreover, national accounts categories are built on the premise that consumer durables are part of consumption. However, from their theoretical viewpoint, they should be included in capital stock, and their flow assigned to GNP. Another important job was to de-trend the data. Business cycles are defined as occurring with a frequency of three to five years around the trend. To isolate them, it is necessary to eliminate lower-frequency movements, related to long-run factors. The most widely used filter in the real business cycle literature is the Hodrick-Prescott filter (Hodrick and Prescott 1980).

As to the model economy, several steps are involved. The first is to assign a precise functional form to production and utility that is consistent with the facts on growth. Adhering to the equilibrium discipline, Kydland and Prescott restricted their attention to model economies that displayed balanced growth. They opted for a Cobb-Douglas production function, integrating a productivity variable, and a utility function restricted to a unitary elasticity of substitution between leisure and consumption. These functional forms are still widely used today. The first choice is justified by the fact that capital’s and labor’s shares of output have been approximately constant in the US in the period since WWII. The second is vindicated by the combination of a roughly constant level of leisure per capita and of a large rise in the real wage rate. Luckily, these two functional forms also happen to be the most tractable ones.

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<sup>14</sup> See Kydland and Prescott (1991; 1996), Cooley and Prescott (2006).

Assigning values to the parameters of the model economy is a crucial step in the program. To this end, Kydland and Prescott resorted to a new methodology, calibration,<sup>15</sup> which differs from econometric testing. This method, they claimed, was already in use in the natural sciences, as well as in computational general equilibrium theory. But, to say the least, they gave it a new impetus. Calibration consists of choosing values for the model economy's parameters either by drawing on existing empirical studies, independent research or national accounting data or by applying economic theory. The more the parameters can be valued in the first way, the better. In their 1982 paper, Kydland and Prescott found themselves with six 'free' parameters still in need of receiving a quantitative value. These pertained mainly to inter-temporal substitution and technology shocks. Different values could be calculated for their various combinations. Kydland and Prescott choose those that resulted in a close correspondence between the moments predicted by the model and those of the real-world series.

The next task is to solve the model. The equilibrium process ought to be computed and simulated to generate equilibrium paths for the model economy, using the recursive competitive equilibrium concept (Stockey and Lucas 1989). Again, this part of the program is central. More often than not, it is a time-consuming activity which turned macroeconomists into computational experts. Finally, after an additional filtering procedure, the researcher can proceed to calculate the selected moments both for the model economy and for the rearranged real-world data and compare them.

When all these steps are completed, a final assessment is possible. Kydland and Prescott's account of their own research runs as follows:

Using the standard neoclassical production function, standard preferences to describe people's willingness to substitute intra- and inter-temporally between consumption and leisure, and an estimate of the technology shock variance, we found that the model economy displays business cycle fluctuations 70 percent as large as did the U.S. economy. This number is our answer to the posed question (Kydland and Prescott 1996, p. 74).

In this description, I have referred to Kydland and Prescott's "Time to build" paper. In subsequent work, things have become easier because of the cumulative development of the approach. However the job to be done is still daunting. Much of it comprises working on the data and making computations, all tasks requiring long hours of tedious calculation.

Merely asserting that qualitative modeling gave way to quantitative fails to convey the full measure of the change that took place. Behind this contrast lies another, more sociological,

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<sup>15</sup> For a presentation of the calibration method see, for example, Wickens (1995), Cooley (1997) or Prescott and Candler (2008). For its criticism see Hoover (1995).



difference. To introduce it, let me remark that, in a certain way, the relationship between Lucas and Kydland and Prescott replicates that between Keynes and his followers. What would have happened to the *General Theory* if its message had not been transposed into the IS-LM model, and if Klein had not extended this model into an econometric framework? While no answer can be provided to such a question, it reminds us that, in a field such as economics, there is no single compelling way in which theory will evolve. The same conundrum arises over the relationship between Lucas, on the one hand, and Kydland and Prescott, on the other. Without Kydland and Prescott, would the seismic change that macroeconomics underwent have occurred? It is far from sure. Lucas's conceptual papers were impressive but too highbrow to generate a huge following. As to Lucas's criticism, its impact on the profession could have been limited to making modelers more cautious when drawing conclusions from their models, and not produced a radical change in method. To have a scientific revolution, an alternative way of doing applied work, providing new grist to the mill for the majority of members of the community, must be made available. This was Kydland and Prescott's main contribution.

Another trait of Kydland and Prescott's approach is that it constitutes a marriage of sorts between Lausanne and Chicago economics, a move that purists of the two schools used to disapprove of. Earlier neo-Walrasian authors were skeptical of, if not opposed to, any direct transposition from a model to reality. To them, their models were evolving at a level of abstraction which made them unfit for empirical testing. For example, Cass, whom Kydland and Prescott hailed as their forerunner, expressed his dissatisfaction with seeing his model transformed in the Kydland and Prescott way (Cass 1998).<sup>17</sup> On the other side of the fence, Friedman, the emblematic Chicago economist, was radically opposed to Walrasian theory.<sup>18</sup> In contrast, neither Kydland and Prescott nor Lucas have any qualms about such a mingling of the two traditions.

Finally, another striking trait of Kydland and Prescott's methodological standpoint is their unshakeable faith in neoclassical theory, that is any model with "agents maximizing, subject to constraints and market clearing" (Kydland and Prescott 1991, p. 164). This is just Lucas's

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<sup>17</sup> Hahn is another example. "It is for all these reasons that I have always held the view that the Walrasian theory in all of its manifestations is an important theoretical benchmark but that a vast and unruly terrain had to be traversed before one understood, let alone predict, the behavior of an actual economy. No economist and certainly no theorist should be ignorant of the Walrasian theory, and no economist and certainly no theorist should pronounce on actual economies and policies on its basis alone" (Hahn 1983, p. 224)

<sup>18</sup> See De Vroey (2009a) for a further discussion of this point.

equilibrium discipline. When it comes to macroeconomics, they view the neoclassical growth model as the ‘established theory’.

I view the growth model as a paradigm for macro analysis — analogous to the supply and demand construct of price theory (Prescott [1986] 1994, p. 266).

Macroeconomics has progressed beyond the stage of searching for a theory to deriving the implications of theory (Prescott 2006 p. 203-4).

Klein constructed his models with the purpose of assessing the validity of Keynes’s ‘hypothetical system’ (Klein 1955, p. 280). True, he had a somewhat biased way of engaging in this exercise by contriving to put the economy in a Keynesian regime with excess supply in the labor and the goods markets. Nonetheless the official aim of the enterprise was to put the theory to the test. This is no longer the case for Kydland and Prescott. The shift that occurred with them is from a methodological standpoint where the validity of a theory has to be established to one where the aim is to apply a well-established theory.

Earlier on, macroeconomists ‘believed’ in neoclassical theory but took it with a large pinch of salt — Solow being a fine example. This was the reason for their adhering to the neoclassical synthesis view. The latter encapsulated the idea that ‘classical’ theory was valid only in the long term, which in turn suggested that a non-classical theory (Keynesian theory) was needed to explain the here and now. This attitude of conditional or mitigated adhesion to neoclassical theory has now disappeared.

## **EXPLAINING THE NC/RBC REVOLUTION**

### **Lucas’s and Prescott’s views**

As we have already seen, in his Nobel lecture, Prescott expresses his pride at having led a revolution that transformed macroeconomics. He described the state of affairs that prevailed before the revolution as follows:

Prior to the transformation, macroeconomics was largely separate from the rest of economics. Indeed, some considered the study of macroeconomics fundamentally different and thought there was no hope of integrating macroeconomics with the rest of economics, that is with neoclassical economics. Others held the view that neoclassical foundations for the empirically-determined macro relations would in time be developed. Neither view was correct (Prescott 2006, p. 203).

To Prescott, the change was mainly methodological. In his eyes, it was so radical that the term revolution is deserved, and so compelling that it could not but win. Beyond these broad

declarations, he devoted little attention to what actually happened. Basically, it was scientific progress in action.<sup>19</sup>

For a more detailed account of the changes that went on, we may turn to Lucas's "Methods and problems in business cycle theory" article ([1980] 1981). He considers three forces that may have been active: technical developments, outside events and the internal development of the discipline. In his eyes, the first of these was most important. He views progress in economic theory mainly as a matter of discovering or applying new tools, new techniques for treating old issues. In effect, one leitmotif of Lucas's methodological writings is that earlier economists felt the need to study the economy in a dynamic way but lacked the necessary tools.<sup>20</sup> Two important transformations took place to change this state of affairs. The first was the possibility of resorting to new mathematical tools, borrowed from engineering, such as control theory. They allowed for the construction of dynamic theory. The second was the increased computational ability associated with the tremendous progress that took place in computer science, paving the way for large-scale simulation work.

As to new developments "thrown at us by the real world" (Lucas [1980] 1981, p. 272), Lucas dislikes giving them too much importance because this would run counter to his premise that all business cycles are basically alike.<sup>21</sup> He nonetheless considers two external influences that may have played a role in the rise of new classical macroeconomics. The first is the stagflation period that characterized the 1970s. To him, it constituted a dramatic quasi-laboratory experiment that confirmed the rightness of Friedman's intuition ([1977] 1981, p. 221). However, Lucas stops short of claiming that this was a decisive cause of the change ([1980] 1981, pp. 282-283). The second is the fact that in the post-World War II period business cycles have followed a regular pattern, giving weight to the view that they are a repeated occurrence of the 'same' event. All in all, however, he sees technical progress as the driving force:

These new observations have been influential (as new observation should be to empirical researchers) but it seems to me that the main outside influences have been, and will continue to be, changes in available theoretical methods. In business

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<sup>19</sup> "The reign of this system-of-equations macroeconomic approach was not long. One reason for its demise was the spectacular predictive failure of the approach. As Lucas and Sargent point out, in 1969 these models predicted high unemployment would be associated with low inflation. Counter to this prediction, the 1970s saw a combination of both high unemployment and high inflation. Another reason for the demise of this approach was the general recognition that policy-invariant behavioral equations are inconsistent with the maximization postulate in dynamic settings. The principal reason for the abandonment of the system-of-equations approach, however, was advances in neoclassical theory that permitted the application of the paradigm in dynamic stochastic settings. Once the neoclassical tools needed for modeling business cycle fluctuations existed, their application to this problem and their ultimate domination over any other method was inevitable" (Kydland and Prescott 1991, pp. 166-167).

<sup>20</sup> See Lucas ([1980] 1981, p. 275), Lucas (1987, p. 2), Lucas (1996, p. 669-).

<sup>21</sup> Lucas admits that the Great Depression remains a "formidable barrier to a completely unbending application of the view that business cycles are all alike" (Lucas ([1980] 1981, p. 273).

cycle theory, it appears not to be the problem that changes but rather the way we look at it. Of changes in methods, certainly the most central have been postwar developments in general equilibrium theory (Lucas [1980] 1981, p. 284).

While the appearance of new tools made the change in approach possible, this change manifested itself in internal theoretical developments. Most of Lucas's paper on 'Methods and problems' is devoted to these. He shows how post-WWII economists relied upon the stationary equilibrium concept. To them, the economy was in a state of disequilibrium as long as it departed from the stationary state. The business cycle was viewed as a manifestation of such a departure. Since disequilibrium was furthermore equated with a mismatch between supply and demand, the main form of which was the Keynesian case of excess supply, the business cycle was deemed to be a market failure. This was how an essentially static theory was supplemented with short-run dynamics, the basic insight of the neoclassical synthesis. According to Lucas, this framework, combining a static long-run equilibrium with short-run disequilibrium dynamics, was used for lack of a better one. This lack in turn was explained by the fact that a series of advances had not yet been made. In particular, possible lessons for the study of the business cycle had not yet been drawn from the Arrow-Debreu model. Once this was done, thanks to Lucas and his associates' contributions, in conjunction with the technical advances that had become available, a radically new framework became possible, wherein "the idea that an economic system in equilibrium is in any sense 'at rest' is simply an anachronism" (Lucas p. 207).

### **An assessment**

On most scores, I can agree with Lucas's interpretation. In particular, his emphasis on the role of new tools Lucas is well taken, an aspect that historians of economics tend to overlook. The story of NC/RBC macroeconomics starts with gifted young economists, who initially found it natural to do research in macroeconomics as it then existed (i.e. working within the Keynesian paradigm), but grew to think that important flaws in this paradigm had been pushed under the rug. Eventually, they came to the conclusion that a radical overhaul was required. Backtracking rapidly, they returned to pre-Keynesian insights — but without rejecting modern analytical methods. They also believed that much could be borrowed from the Arrow-Debreu model and its development. So, the ingredients of the transition to NC/RBC macroeconomics were dissatisfaction with the reigning paradigm, the availability of new technical tools, borrowing from apparently unrelated parts of economics, and a sense of direction — the equilibrium discipline and firm methodological principles, such as the pre-eminence of theory over pragmatism.<sup>22</sup>

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<sup>22</sup> Of course, I am aware that they tell the story from the 'winners' point of the view. The defenders of Keynesian macroeconomics have quite a different view. As Lipsey (2000, p. 76) wrote: "To many Keynesians, the new classical programme replaced messy truth by precise error".

However, I diverge from Lucas's and Prescott's claim that all this was just the natural unfolding of theoretical progress. To Lucas, little is gained by interpreting the changes that took place as a scientific revolution:

Research in my field of specialization — macroeconomics, or monetary and business cycle theory — has undergone rapid change in the past 15 years. One way of describing some of these changes is in terms of ideological contests between rival schools of thought: the 'Keynesian revolution', the 'monetarist counter-revolution', and so on. There is no doubt something to be learned by tracing the main ideological currents in macroeconomic research, but I myself find most of this discussion of crises, revolutions and so on, unintelligible, and almost wholly unconnected with the most interesting current research (Lucas 1987, p. 1).<sup>23</sup>

Admittedly, the notion of scientific revolution should not be applied too freely, as is sometimes done. I, for one, consider the transition from Keynesian to NC/RBC macroeconomics to be the only occurrence of a Kuhnian revolution in macroeconomics to date.<sup>24</sup> While much of what happened can be filed under the heading of 'progress', this does not exhaust the matter. Some aspects of the changes are better viewed as a displacement of perspective than a linear progress. This applies, in particular, to the change in content of the equilibrium discipline, the relegation of unemployment from macroeconomics, and the shift from a Marshallian to a Walrasian framework. Moreover, the rise of the NC/RBC paradigm bears all the sociological hallmarks of a scientific revolution. It started with thundering declarations of war. It triggered a change of guard: the old elite of macroeconomists suddenly became 'has beens' while new figures rose to prominence. All this happened suddenly and rather violently (by academic standards). The title and content of Lucas and Sargent's "After Keynesian macroeconomics" paper ([1979] 1994) is often perceived, and was probably intended, as whistle-blowing. This attitude of not pulling punches is even more visible in the views expressed on circumstantial occasions, as the following extract from a talk given by Lucas at Graduate School of Chicago's Annual Management Conference in 1979 and entitled "The death of Keynesian economics" illustrates:

Keynesian economics is dead [maybe 'disappeared' is a better term]. Don't know exactly when this happened but it is true today and it wasn't true two years ago. This is a sociological not an economic observation, so evidence for it is sociological. For example, one cannot find a good, under-40 economist who

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<sup>23</sup> For all his use of the term 'revolution', Prescott is no more a follower of Kuhn than Lucas is.

<sup>24</sup> I view the 'Keynesian Revolution' less as a revolution than as the first step in the constitution of the new sub-discipline of macroeconomics, a simplified, applied and policy-oriented type of general equilibrium analysis. Nor should the transition towards new neoclassical synthesis models be considered a revolution.

identifies himself, works as a ‘Keynesian’. Indeed, people even take offence if referred to in this way. At research seminars, people don’t take Keynesian theorizing seriously any more — audience starts to whisper, giggle to one another. Leading journals aren’t getting Keynesian papers submitted any more (Lucas 1979).

The question may be asked as to what the ‘value added’ of my standpoint of wanting to combine the opposed intuitions of progress and scientific revolution is? The answer is ‘a grain of relativism’! Kuhn’s notion suggests that any new paradigm that emerges, having dethroned a pre-existing one, will be dethroned in its turn in the future. We may envisage, for example, that one day it will be possible to conceptualize the idea of individual disequilibrium so that the equilibrium approach will become *passé*.

### **A political agenda?**

Is my above picture complete? Possibly not. Look at the following quotation from Blinder:

I argue ... that there was no anomaly, that the ascendancy of new classicism in academia was instead a triumph of *a priori* theorizing over empiricism, of intellectual aesthetics over observation and, in some measure, of conservative ideology over liberalism (Blinder [1988] 1997, p. 110).

Blinder evokes a factor of which no trace is to be found in Lucas’s or Kydland and Prescott’s accounts. Blinder’s accusation is that the NC/RBC revolution, to some extent at least, resulted from the desire to promote a laissez faire approach and to dismiss the more interventionist Keynesian ideology that had previously prevailed.

Clearly, such a line of thought will hardly be popular amongst economists. Most will reject it, be they friend or foe of the NC/RBC revolution. One underlying reason is the tremendous influence exerted by Friedman’s conception of the methodology of economics, asserting its positivistic character. A basic tenant of this view is that value judgments have no place in economic theory.

For sure, it will be difficult to find NC/RBC macroeconomists claiming that they pursue a political agenda. Let me give just two examples of denials of the presence of such a purpose. The first is from Klammer’s interview with Sargent.

[Klammer]: *Are the political aspects an important question?*

[Sargent]: I am not really interested in politics. The rational expectations stuff is clearly not politically motivated. People from all sorts of different political perspectives contribute to it. It’s more a technical revolution. ... No, it’s certainly not politically motivated (Klammer 1984, p. 80).

My second example is Kehoe and Prescott's reply to Temin's (2008) criticism of their work in the *Journal of Economic Literature*. One of Temin's indictments was that real business cycle authors were pursuing a political agenda. Kehoe and Prescott's reacted by writing:

We can assure Temin and readers of our book that there is no agenda in the book but a scientific one. The suggestion that we have such an agenda is yet another example of Temin's lack of understanding of the book (Kehoe and Prescott 2009, p. 21).<sup>25</sup>

I have no reason to question the sincerity of Sargent, Prescott, and other authors in the NC/RBC tradition. But for several reasons that does not close the question. First of all, returning to square one, we must remember that political economy started with Adam Smith as a plea in favor of a system of economic liberty. In the *Wealth of Nations* positive and normative aspects are intertwined. Still today, an important part of the theoretical conversation among economists bears on whether the invisible hand is doing its job. This is true, not for all fields of economics, but for those that deal with the economy as a whole, as is the case with macroeconomics. The issue at stake is how the market economy can best work — with or without state intervention, and, if with, to what extent? If an opinion about the ideal organization of the economy is ideological, then ideology cannot be absent from macroeconomics. To me, this is not a stigma. It just means that economics is less remote from political philosophy than is usually believed.<sup>26</sup> As to Friedman, for all my admiration for him, I find it difficult to swallow that he was a value-judgment-free economist. All his life he passionately pleaded the cause of *laissez faire*, using every type of argument he came across.

What are the possible ideological views in presence in macroeconomics? In a recent paper (De Vroey 2009b, written in French) I have distinguished seven degrees of economic liberalism with at its top 'Austrian economic liberalism' and at its bottom 'communism', complete state control of the economy. As far as macroeconomics is concerned, the spectrum can be limited to two levels, the '*laissez faire*' and the 'Keynesian' conceptions. The 'Keynesian' modifier can be viewed as a catchword for grouping those authors who, although generally supportive of the market system, nonetheless believe that it can exhibit failures which state intervention, in particular demand stimulation, can remedy. Here is Blinder's account:

A normative Keynesian believes that government should use its leverage over aggregate demand to reduce the amplitude of business cycles. He or she is probably

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<sup>25</sup> Lucas lapsed at least once into taking a more relativistic standpoint when he said in an interview that: "In economic policy, the frontier never changes. The issue is always mercantilism and government intervention vs. *laissez faire* and free market" (Lucas 1993: 3).

<sup>26</sup> My understanding of the term 'ideology' is relativistic, unlike its common-sense understanding which is often pejorative, pointing to the alleged bad faith of political opponents.

far more interested in filling in cyclical troughs than in shaving off peaks. These normative propositions are based on judgments that (a) macroeconomic fluctuations significantly reduce social welfare, (b) the government is knowledgeable and capable enough to improve upon free-market outcomes, and (c) unemployment is a more important problem than inflation (Blinder 1988: 112–3).

The laissez faire (or anti-Keynesian) view is that the unfettered working of competition will lead the economy to the best attainable position. To repeat a well-known aphorism, the state is the problem, not the solution. Stabilization policies are neither necessary nor efficient.

A second factor that casts doubt on the non-ideological character of macroeconomics appears when looking at the policy conclusions supported by the main models that have succeeded each other over the history of macroeconomics. The results are summarized in Table 1.

Table 1. The policy cause defended

	Justifying demand activation	Laissez-faire
The <i>General Theory</i>	♦	
The IS-LM model	♦	
Disequilibrium theory (Patinkin and Leijonhufvud) and non-Walrasian equilibrium models	♦	
Monetarism		♦
New classical models (Lucas)		♦
New Keynesian models	♦	
Real business cycle models (Kydland & Prescott)		♦
The new neoclassical synthesis	♦	♦

The to-and-fro process illustrated in the table looks incompatible with the hypothesis of an unwavering march towards increased knowledge based exclusively on conceptual, technical and factual advances. Some additional factors must have played a role. Political agendas are a candidate.

Finally, a third argument is that the policy conclusions which a given theory or model can support are constrained by their premises. According to the type of model adopted, certain results are possible, others excluded. To wit, an elementary Walrasian model with complete markets and without externalities excludes coordination failure and states of underemployment. Of course, researchers can enter into a theoretical framework that has policy implications without having a political motivation. More importantly, they can be blind



to the predetermined character of the policy conclusions of the model they use. Let me refer again to Kehoe and Prescott's answer to Temin. Having stated that they are engaged in science and not politics, they make the following statement:

The underlying hypothesis of the book is that the general equilibrium growth model is a useful tool for studying great depression episodes. The tentative findings are that bad government policies can turn ordinary economic downturns into great depressions. These finding are especially relevant now, late 2008 (Kehoe and Prescott, 2009, p. 21).

Following in Friedman and Schwartz's footsteps, Kehoe and Prescott view government failures (i.e. mistaken policy measures) as responsible for great depressions. They may be right, but the problem is that this conclusion is worth little if it is based on a model that excludes any other causes, in particular trading malfunctioning, in an *a priori* way. To really determine the reasons for great depressions we should have models that consider all the main suspects. In other words, as soon as an equilibrium model is used, the dice are stacked. I admit that, at present, disequilibrium models are too hard to construct. In this situation, two attitudes are possible. The first is to admit to the limitations of the type of model employed, and, as a result, to avoid drawing definite conclusions. The second, taken by Kehoe and Prescott, is to be oblivious of the implications of the approach, and to voice blunt conclusion. To me, this is a methodological fraud, a case of theoretical hubris, and it has an ideological ring. This is possibly what Blinder and Temin had in mind.

It is thus well worth pondering the role of ideology in the NC/RBC revolution. The challenge is to be able to combine the statements made by its leaders (that they had no political motivation) and the presence of an ideological dimension in their approach. One view that needs to be discarded from the outset is that all practitioners of macroeconomics are aware of its ideological dimension. Most of them are mere foot soldiers in the regiment. They march in step without any need to be aware of where the regiment is coming from or where it is heading. This being said, most macroeconomists do have ideas about the best economic policy. So, individually, a large number of them could place themselves on the spectrum from *laissez faire* to Keynesian; but often this has no impact on their theoretical practice. There may be exceptions, people who consider themselves pure technicians. Some may change their minds over time (possibly because of the study of economics). None of this matters for my claim.

Nor do I want to claim that every theoretical move should be viewed as the result of a political agenda. The starting point for engaging in theoretical innovations under the impulse of a political agenda is finding that the prevailing paradigm leads to policy conclusions that you dislike. But these are unusual situations. Only a few episodes in the history of macroeconomics, to my mind three, qualify. We have Keynes, who wanted to amend what he

called classical theory; Friedman, who wanted to recast Keynesian macroeconomics in order to reach *laissez faire* conclusions; and New Keynesian economists who wanted to import Keynesian themes into the new theoretical discourse introduced by new classicists and real business cycle theorists.

Note that I have not included either the new classical or the real business cycle episodes in this category of politically-motivated theoretical moves (let me repeat that I have nothing against these). The reason is that Lucas, Sargent, Kydland and Prescott did not need to act upon a political agenda, because Milton Friedman had already carried out much of the job of gearing theory towards a political agenda other than the Keynesian!

Friedman had a political agenda and used macroeconomics in a way that was not conceptually revolutionary but marked a radical shift in terms of policy conclusions. Economists like Lucas and Sargent boarded the train after it had left. While they held the same *laissez faire* ideology as Friedman, they no longer needed to promulgate it. Rather they could concentrate on working, as pure technicians, on the conceptual and technical modifications that were necessary to provide firm ground for Friedman's policy conclusions. In other words, a division of labor occurred, with Friedman doing the political job, and the next generation undertaking the theoretical developments needed to vindicate the political agenda inaugurated by Friedman.

## CONCLUDING REMARKS

In this paper, I have tried to make the following points. First, I have criticized the standard understanding of the requirement for microfoundations, and argued that a softer understanding of it, admitting states of individual disequilibrium, is more appropriate. Unfortunately, theories based on it are still non-existent. The NC/RBC revolution can be described as a transition from the soft to the demanding understanding of this requirement. It implied the *a priori* expulsion of market non-clearing from macroeconomics. Second, I have characterized the new classical stage of the revolution as having introduced a series of novelties: a merger of the fields of value theory and business cycle theory that were previously separate, the expulsion of unemployment from the agenda of macroeconomics, the replacement of Marshallian by Walrasian macroeconomics. Third, I have shown how Kydland and Prescott made Lucas's qualitative model quantitative. I have suggested that if this had not been done, a scientific revolution *à la* Kuhn might not have occurred. Fourth, my study Lucas's and Kydland and Prescott's accounts of the NC/RBC shows that, although Prescott used the term 'revolution', what these authors had in mind was just the uninterrupted march of progress, in contrast to my own interpretation and a far cry from a Kuhnian perspective. Fifth, I have addressed the challenge of conciliating the assertion made by the leaders of the NC/RBC revolution that they were doing science without a political agenda with the fact that

laissez faire policies are a foregone conclusion in their models. This has been possible, I have argued, because the job of putting forward the laissez faire political agenda had previously been carried out by Friedman, so that his disciples could devote themselves to the task of solving theoretical puzzles and doing applied work.

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